country where the rocks are moderately folded, and which was insisted on and fully illustrated in a manual of geology published fourteen years ago, and then could hardly have been said to be new. The distinctive characters of mountain chains are treated of in the same spirit. Little, if anything, is here added to the masterly summary of these characters which Dana gave us years ago, but much space is taken up with illustrations of truths which every one admits, in the form of long quotations from sundry sources. All this speaks to much reading and patient industry, and we cannot but admire the conscientious care with which Mr. Reade has striven to inform himself of all that has been said and done on the subject of which he is treating; but we cannot help asking ourselves whether it was necessary to print at length the contents of his note-books. This accumulation of evidence, where it is not needed, has brought with it an attendant evil: it has swelled the book to an undesirable size. Now in these busy days, and in the interest of readers, if there is one thing against which, more than anything else, a resolute stand ought to be made, it is unnecessary printing. The day is yet far distant when every page of printed matter shall contain something that is new, and nothing that is not new; but this is the impossible ideal, the asymptotic consummation, which all writers should ever keep prominently in view.

But we would not press this point, because, even if the author has given us rather an excess of matter that is not new, what he has given is good of its kind; and it is of more importance to weigh his arguments against the contraction hypothesis, and in favour of the Scrope-Babbage explanation with the additions he suggests to it. Holding as he does that expansion by heat is the main factor in producing the disturbances of the earth's crust, he made experiments to determine the coefficients of expansion of sundry rocks. He arrives at a mean which agrees very nearly with that found by Mr. Adie. Mr. Reade's experiments ranged from temperatures of 60° to 220° F., and he assumes that the coefficient of expansion will be the same for the enormously higher temperatures with which he has to deal when considering the case of the earth—a risky proceeding, to say the least. But he has fallen into a far more serious mistake: he has assumed that rocks weighted with a thickness of twenty miles of overlying strata will expand to the same extent for a given increase of temperature as rocks under atmospheric pressure. The oversight involved in this assumption so thoroughly vitiates all his numerical results that no conclusion can be drawn from them.

In Chapter XI. we have the objections to the contraction hypothesis succinctly stated. The numerical results we put aside for reasons just given, but in his general argument the author does not seem to us to realise the full meaning of the hypothesis. He seems to hold that according to the contractionists crumpling is produced by unequal contraction *in the solid shell itself*, which certainly is not their view. And he entirely omits all reference to the one fact which is the life and soul of the hypothesis, that the earth's crust is not strong enough to stand by itself without support, a fact which admits of rigid mathematical demonstration. It is a decided case of a seriously mutilated representation of the play of "Hamlet."

We cannot therefore admit that Mr. Reade's arguments are very damaging to the hypothesis against which they are directed; and we cannot see how expansion due to rise of temperature could alone produce the results which he attributes to it. The strains produced in this way would tend to be relieved by yielding in the direction of least resistance—that is, vertically; and if there were no impediment to the perfect transmission of strain, the yielding would be wholly in this direction. In the actual case a certain amount of deformation would doubtless be produced within the heated mass itself, but hardly enough, it would seem, to cant over a huge anticlinal, and lay it nearly flat on its side. The machinery invoked by the contraction hypothesis may or may not have been the means by which such overthrusts were brought about, but it is the only machinery yet suggested which seems competent to produce them. "Seems," we say throughout, for the question between rival hypotheses is as yet only one of probability.

And we think no one will contend that the Scrope-Babbage hypothesis ought to be entirely put on one side when we speculate on the cause of earth-movements; great broad folds, such as those of the plateau-region of Utah, described by Capt. Dutton, and figured on Plate 39 of the book, may have been caused by the bulging up of heated masses below, though they can be explained equally well by the contraction hypothesis.

It remains to notice that the book is rich in figured illustrations. A number of the plates are devoted to somewhat diagrammatic landscapes of contorted rocks, and these bring out well the points they are intended to illustrate; but they do not add to the stock of our knowledge. They would be serviceable to a geologist, if such a one there be, who had never stirred out of the fen-country, but he of course would do still better if he took an excursion ticket to some of the localities from which the views are taken.

A. H. Green

ORGANIC EVOLUTION

The Factors of Organic Evolution. By Herbert Spencer. (London and Edinburgh: Williams and Norgate, 1887.)

MR. HERBERT SPENCER has done well to reprint in a permanent form his two articles on the "Factors of Organic Evolution," which were published last year in the *Nineteenth Century;* for, although they present substantially the same doctrines as are to be met with upon this subject in his "Principles of Biology," they do so in the light of fuller knowledge and more matured judgment.

The object of the essay is that of taking stock, so to speak, of natural selection as compared with other "Factors." Mr. Spencer's treatment of this subject is admirable, and ought to be read by all working naturalists who have any interest in the problems of evolution. The literature of Darwinism has now become so extensive that even first-rate naturalists who are engaged on other lines of work are apt to get left behind, or, with respect to Darwinism, themselves to become examples of what are now called "vestiges": their ideas are the superseded survivals of some previous phase of evolutionary science. And most of all is this true with regard to the fundamental question so ably discussed by Mr. Spencer. As he remarks, "Nowadays most naturalists are more Darwinian than Mr. Darwin himself," by which he means that most naturalists attribute more to the agency of natural selection than was attributed to it by the final judgment of its discoverer. The reason of this is that most naturalists have neither read with any care the later editions of Mr. Darwin's works, nor probably even so much as heard of the sundry essays which led him to modify his views upon the comparative importance of natural selection and other factors of organic evolution. Such naturalists, therefore, are not true Darwinians. Still believing in natural selection as almost the only factor of organic evolution, they are archaic enough to suppose that distinctions of specific value are almost universally of an adaptive kind. In this respect, indeed, they share what is no doubt still the popular impression of Darwinism, but an impression, nevertheless, which does a great injustice to the genius of their master. In order that there may be no mistake upon this matter, we will here supply a few quotations from the latest editions of Mr. Darwin's works, over and above the numerous extracts which Mr. Spencer has selected for the same purpose.

"I now admit, after reading the essay by Nägeli on plants, and the remarks recently made by various authors with respect to animals, more especially those recently made by Prof. Broca, that in the earlier editions of my 'Origin of Species' I perhaps attributed too much to the action of natural selection, or the survival of the fittest. I have altered the fifth edition of the 'Origin' so as to confine my remarks to adaptive changes of structure; but I am convinced, from the light gained during even the last few years, that very many structures which now appear to us useless will hereafter be proved to be useful, and will, therefore, come under the range of natural selection. Nevertheless, I did not formerly consider sufficiently the existence of structures, which, so far as we can at present judge, are neither beneficial nor injurious; and this I believe to be one of the greatest oversights as yet detected in my work." ("Descent of Man," 2nd edition, p. 61. He goes on to explain how he was led to the "tacit assumption that every detail of structure, excepting rudiments, was of some special, though unrecognised, service," and concludes by remarking that "any one with this assumption in his mind would naturally extend too far the action of natural selection.")

"In the earlier editions of this work I underrated, as it now seems probable, the frequency and importance of modifications due to spontaneous variability" ("Origin of Species," 6th edition, p. 171). "It appears that I formerly underrated the frequency and value of these latter forms of variation, as leading to permanent modifications of structure independently of natural selection. But as my conclusions have lately been much misrepresented, and it has been stated that I attribute the modification of species exclusively to natural selection, I may be permitted to remark that in the first edition of this work, and subsequently, I placed in a most conspicuous position—namely, at the close of the Introduction—the following words: 'I am convinced that natural selection has been the main but not the exclusive means of modification.' This has been of no avail. Great is the power of steady misrepresentation; but the history of science shows that fortunately this power does not long endure" (*Ibid.* p. 421).

"When, from the nature of the organism and of the conditions, modifications have been induced which are unimportant for the welfare of the species, they may be, and apparently often have been, transmitted in nearly the same state to numerous, otherwise modified, descendants. . . . A structure, whatever it may be, which is common to many allied forms, is ranked by us as of high systematic importance, and consequently is often assumed to be of high vital importance to the species. Thus, as I am inclined to believe, morphological differences, which we consider as important—such as the arrangement of leaves, the division of the flower or of the ovarium, the position of the ovules, &c.—first appeared in many cases as fluctuating variations, which sooner or later became constant through the nature of the organism and of the surrounding conditions, but not through natural selection; for, as these morphological characters do not affect the welfare of the species, any slight deviation in them could not have been governed or accumulated through this latter agency" (*Ibid.* pp. 175, 176).

These quotations are added to those which have been supplied by Mr. Spencer, in order still further to advance the "motive" with the expression of which his essay concludes. After directing attention to the present views of Prof. Huxley upon the subject—viz. "How far natural selection suffices for the production of species remains to be seen"; and "Science commits suicide when it adopts a creed"—Mr. Spencer closes with the following remarks:—

"Along with larger motives, one motive which has joined in prompting the foregoing articles has been the desire to point out that already among biologists the beliefs concerning the origin of species have assumed too much the character of a creed; and that while becoming settled they have become narrowed. So far from further broadening that broader view which Mr. Darwin reached as he grew older, his followers appear to have retrograded towards a more restricted view than he ever expressed. Thus there seems occasion for recognising the warning uttered by Prof. Huxley as not uncalled for. Whatever may be thought of the arguments and conclusions set forth in this article and the preceding one, they will perhaps serve to show that it is as yet far too soon to close the inquiry concerning the causes of organic evolution."

Of these two articles the first is devoted to a consideration of use and disuse as causes of such evolution, while the second treats of the influence of surrounding conditions. The latter is the more highly speculative, and therefore may be here considered in fewer words. The idea is that all external parts of organisms, being exposed to different physical conditions from the internal parts, must be differently affected thereby: natural selection apart, there must here be recognised the differentiating agency of a direct or purely physical kind. Hence a certain rough analogy is drawn between a cell-wall, or cuticle, and the oxidised exterior of an inorganic body. Many of the differentiations undergone by the epiblast in the course of organic evolution may, it is argued, be best explained by the immediate action of external agencies—just as we know that a surface of mucous membrane, when brought into permanent relation with such agencies, changes from cylinder epithelium to squamous epithelium: "the effect of the medium is so great that, in a short time, it overcomes the inherited proclivity, and produces a structure of opposite kind to the normal one." Many other examples of the same general principle are given; but the essay as a whole is ingenious rather than convincing. Not, of course, that we dispute the principle—which, indeed, is recognised by Mr. Darwin, in one of the passages above quoted, and elsewhere—but it is of too general a kind to admit of being clearly traced in particular instances, where we have to do with all the other elements in the complex of living material.

The main question, however, with which Mr. Spencer is concerned is as to the place which should be assigned to use and disuse as factors of organic evolution. This long-standing question is one of fundamental importance to the whole philosophy of evolution; and as it has now reached a critical phase, the publication of Mr. Spencer's essay furnishes a fitting opportunity for considering its present position in all its bearings. This, therefore, we shall endeavour to do at an early date, in the form of a general article dealing with all the more important literature upon the subject. GEORGE J. ROMANES

TEXT-BOOK OF BRITISH FUNGI

An Elementary Text-book of British Fungi. By William Delisle Hay, F.R.G.S. Royal 8vo, cloth, illustrated. (London: Swan Sonnenschein, Lowrey, and Co., 1887.)

WE have little sympathy with such publishers as produce books written "to order" for the purpose of utilising illustrations, or matching. Still, we cannot help feeling some sympathy with, and pity for, the poor unfortunate who is called upon to perform such an unthankful office as the preparation of "copy." In so far as the book before us corresponds with such conditions our author commands our sympathy, perhaps he deserves it, for even in the preface he seems to fall on his knees, and sue, *in formâ pauperis*, for pity from readers and critics alike. There is doubtless a history connected with this volume. The woodcut blocks which accompany the text, but do not illustrate it, formerly did duty in the "Hand-book of Fungi" published more than fifteen years ago. In the way of business they were transferred, and, in order to utilise them, the "Text-book" seems to have been projected. There are 64 pages of figures and 238 pages of letterpress, but only about 16 pages of the plates have anything to do with the letterpress, and are not even mentioned, so that there are no less than upwards of 40 plates which are supplementary to the letterpress, and have nothing whatever to do with it, except to increase the bulk of the book. We do not know what purchasers would expect to meet with in "an elementary text-book of British fungi," but we suspect that they would scarcely be satisfied with a "treatise on edible and poisonous fungi," or, as set forth in the preface, an attempt "to cover as comprehensively and accurately as possible the entire subject of fungi, considered as aliment." Indeed, it would have been more correct to call this "An Elementary Text-book of some Species of British Fungi."

The professed object of this book, in so far as its writer was concerned, was to present a guide, which above all things should be safe and trustworthy, on the subject of fungus-eating—"so far as toadstool-eating goes," he writes, "I believe I have a right to speak with authority"—and hence if this professed object is not attained, the book must be confessedly a failure. "For the most part," he says, "subsequent authors have added little to what Dr. Badham had advanced." The inference must be that he was ambitious of making a considerable advance, "directed and inspired by a wide acquaintance with mycological literature." After this spontaneous confession it is surprising to find him stating as a fact that "some four hundred and fifty species of the genus *Agaricus* are recorded as occurring in Great Britain," on the faith of a work published now sixteen years ago; his knowledge of mycological literature having stopped short of the fact that nearly 700 species have already been figured, and the latest work on British fungi records 782 species as British, so that the total is nearly double that which his "wide acquaintance with mycological literature" had revealed to him. Leaving, however, such trivial details, let us turn to the "Comprehensive Catalogue of Esculent British Fungi." The first thought is naturally one of order. To assist in reference, and in producing favourable results, one would have hoped to see some method in the grouping of the 221 supposed esculent species. Scientific method there is none, for the white-spored, pink-spored, and brown-spored Agarics are jumbled together in glorious confusion. This would be tolerable if in compensation the species had only been grouped in sequence, or in sections according to their esculent value, but even this has not been done. In two or three instances, however, the species of a given genus are classed in the alphabetical order of their specific names.

Space will not permit of our remarking upon all the individual species included in this miscellaneous list of fungi recommended for common consumption by one who "believes he has a right to speak with authority." For ourselves, we should not have recommended *Agaricus asper*, for with considerable experience we do not remember to have met with it but once in thirty years; nor *Agaricus cæsareus*, for we venture to declare that it has never been found in the British Islands at all, and the same must be said of *Polyporus corylinus* and *Polyporus tuberaster*. Why, then, are they included in a "Catalogue of Esculent British Fungi"? Worse still—because far more dangerous—why are such species included as *Agaricus sinuatus*, which nearly killed Mr. Worthington Smith, and *Lactarius piperatus*, *Lactarius turpis*, and *Lactarius torminosus?* If there are such things as dangerous fungi at all, these are of them. If this writer is really acquainted with these species, and pronounces them edible, let him eat them and enjoy them, but not recommend them to an unsuspecting public. It is our firm and conscientious belief that a book which seriously recommends such things as articles of food might produce calamitous results, if widely circulated, or even, if not, in the event of other books or journals repeating upon its authority that these may be eaten. We read in the preface these significant words: "It has never been my privilege, as yet, to meet with any person versed in mycology from whom I could derive instruction." When the writer meets with such persons we advise him to propose to them an experiment in eating *Lactarius piperatus*, or *Lactarius torminosus*, or even *Agaricus sinuatus*, and we rather fancy that such mycologists will have but little faith in his practical mycology. We sincerely hope that he will not meet with "persons versed in mycology" for the first time in a coroner's court, over a case of poisoning induced by his recommendations. It would have given infinitely more satisfaction, and been far safer for his reputation,